for Darwin, whilst against them were ranged L. Agassiz, Le Conte, Hunt, Semper, and Rein. One might with equal justice claim for an eminent engineer that a bridge constructed by him as a young man in 1842 and repaired by him when an elderly man in 1874, represented the accumulated experience of a long professional career, notwithstanding that he had never since erected another bridge, and had devoted all his time to works of a very different character, and in spite of the fact that, whilst only two engineers of reputation had in the interval pronounced the bridge to be safe, five others of equal eminence had advised that, owing to its rickety condition, it should be pulled down. If it was a question of personal safety, most people would trust to the opinion of the many.

I will first clear the way by laying stress on the circumstance that neither A. Agassiz nor Murray doubt the reality of subsidence. They only contend that the characteristic form of atolls and barrier reefs is not dependent on it, and that such reefs would be produced by their natural mode of growth alone.

Now, with regard to Fiji, Captain Moore is perhaps not aware that in the writings of Murray, Semper, Geikie, and others, in these columns and elsewhere, the association of the three classes of reefs in the same group, as in Fiji, has often been referred to as opposed to the theory of subsidence. It was the occurrence of all reefs in the Fijis and in the Pelew Group, and the ascertained existence of upraised reefs in the last locality, that amongst other reasons led Murray and Semper to find some different explanation than that of subsidence. Dana saw the difficulty, and tried to avoid it by asserting that small fringing reefs are "often evidence of subsidence, even a greater subsidence than is implied by barrier reefs" (*Amer. Journ. Science and Arts*, 3rd series, vol. viii., p. 316). Semper, however, pointed out that this conclusion at once destroyed the value of the testimony afforded by coral reefs of the movements of the earth's crust, since, if all kinds of reefs can be formed during subsidence, the character of a reef cannot guide us in determining the existence of subsidence or upheaval ("Animal Life," p. 233). Darwin also saw the difficulty of the Pelew Group, where living atolls, barrier reefs, and fringing reefs were associated with upraised ancient reefs; and thus it came about that, when writing to Semper in 1879, he generously observed that, although he still adhered to the theory of subsidence, such cases as that of the Pelew Islands, if of at all frequent occurrence, would make his conclusions of very little value. "Future observers," he went on to say, "must decide between us," &c. (Semper's "Animal Life," p. 456).

I contend that in 1889 Mr. Darwin's condition has already been realized, since we have in the interval proved that in respect to the occurrence in the same locality of all three classes of coral reefs and their association with ancient upraised reefs, the Fijis, the Tongan Group, and the Solomon Islands are but reproductions of the Pelew Group. It will therefore be seen that Captain Moore is in error when he believes that Darwin would not have regarded simultaneous up and down movements in the same group as inconsistent with his theory. Darwin's admission to Prof. Semper in the case of the Pelews shows plainly enough what he would have thought of the Fijis. This difficulty of imagining simultaneous up and down movements in the same group was also perceived by the present Director-General of the Geological Survey, when he remarked some years ago in these columns that "such an association of upheaval with an assumed general subsidence requires, on the subsidence theory, a cumbrous and entirely hypothetical series of upward and downward movements" (NATURE, vol. xxix. p. 107; Proc. Roy. Phys. Soc., viii.).

It is somewhat remarkable that long as the Fiji Group has been known, it is only of recent years that anything has been published concerning its upraised coral reefs and its other evidences of considerable upheaval. Darwin, as late as his edition of 1874, knew nothing of the extensively upraised reefs, and he still coloured the group in his map as in an area of subsidence. Strangely enough, amongst the many errors perpetuated in the edition published in the present year of Mr. Darwin's work, is that relating to this group. In an additional footnote (p. 215) we find no reference to the ancient coral reefs upraised some hundreds of feet in the Lau or Windward Group, which are referred to in the Hydrographic publication of 1882 concerning Fiji, and which were described to me by Lieut. Malan some years ago. We find no reference in this edition of 1889 to the conclusion of Mr. Brady that the Suva soapstone, as indicated by its Foraminifera, was formed in depths of from 150 to 200 fathoms in post-Tertiary times, thus implying an upheaval of from 900 to 1200 feet (Geol. Soc., November 9, 1887). Curious as it may appear, Dana, after his lengthened stay in the group, found proofs of an elevation in the larger islands of only 5 or 6 feet, and was inclined to negative it altogether in the case of the eastern islands ("Corals and Coral Islands," 1872, pp. 342, 346).

Captain Moore refers to the "many evidences of upheaval" in Fiji, and instances the occurrence of shells and coral "at great heights." What evidence has he of subsidence? He points to the form of the reef in various islands, and thus assumes the very question at issue. However, I will leave to Mr. Murray to explain how these reefs attained their characteristic form without the assumed movement of subsidence, of which in fact we can find no direct proof.

H. B. GUPPY.

AS Dr. Guppy asks for information with regard to the corals found living at the greater depths round islands in the Indian and Pacific Oceans the following may be of interest.

About two years ago Capt. Wharton, F.R.S., called attention to the Tizard Bank, and last year both it and the Macclesfield Banks were examined by H.M.S. *Rambler*.

The further investigation of the material sent home has shown that the large number of eighteen genera with forty species were found living in depths from 20–44 fathoms outside the reefs, and these species differed with but few exceptions from those in the shallow water.

The following ten genera were found at a greater depth than 30 fathoms—Stylophora, Astræa, Pavonia, Cycloseris, Leptoseris, Stephanaria, Psammocora, Montipora, Alveopora, and Rhodaræa, besides seven small scarcely reef-building genera.

The total number of species collected was 142, so that nearly one-third were represented in over 20 fathoms.

P. W. BASSETT-SMITH, R.N.

Hibernation of Martins in the Argentine Republic.

PROF. CARLO SPEGAZZINI, an Italian botanist, and quite a trustworthy observer, living at La Plata, the new town in the Argentine Republic, writes from there the following account to my friend the Marquis Giacomo Doria of Genoa:—

"The bird known here by the name of *Golondrina*, and which I think is *Progne domestica*,[1] is subject to hibernation. Last year, while the zinc roof of a small house was being taken up in the month of August, just in the middle of our winter, I found underneath about a hundred martins, all accumulated one over the other and lethargic, but in good health, so that, exposed to the sun, they awoke and flew away very briskly. This year, again, having seen some holes on a *barrancha*, a steep bank over the Plata, I began to dig at them, hoping to find some bats; but there I found several hundreds of the martins of the same kind as above mentioned, clustered and in a state of lethargy. Is such a thing known to naturalists?"

TOMMASO SALVADORI.

Zoological Museum, Turin, June 18.

Atmospheric Electricity.

THE interesting accounts of certain electrical phenomena of the atmosphere in NATURE of May 16, 23, and 30, lead me to state that it is a common experience of surveying parties, especially on the high peaks and slopes in the western part of this country to undergo these peculiar electrical sensations. In general these may be described as tingling or pricking sensations, accompanied with hissing or crackling sounds, especially marked if a finger be presented to any metallic object near by. But further than this it has been noticed that whenever a flash of lightning occurs there is a sudden cessation of the distressing electrical effects. The explanation of this is, we think, found in some experiments made at the top of the Washington Monument (elevation 500 feet) during thunderstorms. With a "water dropper collecter," Mascart insulators and quadrant electrometer, we measured the difference of the electrical potential of the air and the ground. The electrometer needle becomes very active with the approach of the "thunder-heads," and after considerable oscillation begins to move steadily in one direction as if subjected to a steadily increasing "pull," and then suddenly, when a potential difference of several thousand volts may be indicated, there is an "instant" drop to zero, and apparent rebound in the other direction, not due to the torsion of the

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[1] Or more likely *Progne chalybæa*, Gm.—T. S.

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suspending wire-fibre. The drop to zero is simultaneous with each flash of lightning. So certain is this relation that we can time the lightning flashes without seeing them. After the flash the needle begins to again move in one direction, repeating its previous behaviour, so that our electrometer measurements seem to prove that every flash of lightning relieves the state of stress of the air, which we may compare with the glass in a Leyden jar, the cloud and ground being the respective coatings.

We may also get at the same result by noticing the effects of the electrification of the dust, smoke, water, and other matter in the air. Whenever our "collector" was "grounded," the fine stream of water issuing from it preserved a certain even rounded form, breaking into drops some four inches away from the place of exit. Removing the "ground" connection, and the stream being now under the influence of the thunder-clouds, the steadily increasing electrification shows itself in the stream's twisting and splitting into innumerable threads and spray; but with each flash of lightning the distortion instantly ceases, and the stream has its normal character, only to be again distorted.

For the benefit of those wishing to photograph lightning I suggest this as a cheap and easy mode of getting warning when to expose. A small tin case with a nozzle giving a fine thread of water or any form of the "burning match" device, well insulated from the ground, and at some elevation, will indicate by changes in the character of the stream or smoke, the approximate degree of the electrification of the air.

New York, U.S.A., June 20. ALEXANDER MCADIE.

Upper Wind Currents over the Equator in the Atlantic Ocean.

REFERRING to the remarks of the Hon. Ralph Abercromby on the above in NATURE of May 30 (p. 101), I would ask for the longitude and latitude for the two crossings of the doldrums, to enable one properly to follow, and eventually work out, the facts. For if our famous meteorologist, on the outer journey, passed within one hundred miles of the West Coast of Africa, the great chain of desert lands, extending many hundreds of miles through Asia to the Sahara in the main weather thoroughfare, would, by its influence, very much contract the width of the calm belt, and otherwise draw the doldrum much north of the line to what would be found more to the westward, where, as it neared the American coast, the breadth of the doldrum belt would very greatly increase; so that, unless the doldrum was crossed at the same longitude, the varying atmospheric conditions should not be put down solely to the sun, or difference of December and May seasons.

Perhaps it is from want of the longitude that I am unable to understand "how low clouds from the south-east drove over north-east trades up to 15° north." All else is exceedingly satisfactory with the law of winds, on the supposition that the return crossing of the doldrum belt took place some hundreds of miles further west, and about half that distance farther south, than on the out journey.

If I may be allowed to digress a little, I would refer to the splendid travels by your correspondent, as published last Christmas under the title of "Seas and Skies in many Latitudes," which in some respects, I think, may be compared with the celebrated *Challenger* Expedition. But, singularly enough, while I am able to follow and accept all the author's research and information, I find myself diametrically opposed to his conclusions. Thus, to quote from p. 428, we have: "Hence we see the proof of the assertion that the trades and monsoons do not meet and force one another to rise, and flow back poleward, but that the two winds coalesce and form one great eastern current over the doldrum." To my mental capacity all the proof is the other way about. If the trades of both hemispheres do not ascend over the calm belt, what other escape or vent is there for them? I am aware that the Meteorological Section of the Krakatão Committee of the Royal Society also favour the idea of a great easterly current ever going west at a certain altitude over the doldrums, but so far as I can find out they do not tell us whence it cometh nor whither or how it goeth. In its circuit going west, we naturally look for its return from the east, and with the constant arrival of fresh winds *viâ* the trades of both hemispheres, it must accumulate if it does not flow back poleward quite as fast as it arrives, for we cannot entertain annihilation of atmosphere any more than of matter. If some other way of escape could be found, we should still have to face the question, Whence comes the supply to the "trades if not from the poles, and whence our prevailing south-westerly winds if not from over the doldrums"? E. FOULGER.

Liverpool, June 18.

P.S.—I do not call in question "the great easterly current over the doldrum," but rather consider its discovery as extremely interesting, it being just what a small school of meteorologists would expect; and it now appears to be left for them to supply a theory for the cause of the direction and also of the motive power of such an atmospheric passage, and possibly for that of the Krakatão dust. E. F.

Patches of Prismatic Light.

I AM curious to know if any of your readers observed the following phenomenon in the sky, and could give any information as to its nature.

When driving with two friends on Saturday evening, June 22, between 6.30 and 7 p.m., in the neighbourhood of Glatton, near Peterborough, we observed on either side of the sun (the sky being almost cloudless) two patches of prismatic light; they appeared to be of nearly the same size as the apparent disk of the sun, and distant from it a hand's span measured from little finger to thumb at arm's length. At the time there were a few light clouds about, but the prismatic patches were not projected on them, as the clouds passed occasionally in front of them, the patches meanwhile shining through the thinnest parts, and reappearing when the clouds had passed, clearly standing out against the sky. There was no appearance of a continuous arch, as in a rainbow, and, unlike a rainbow, the patches were on the same side of the sky as the sun. I may add that the phenomenon was seen by all three of us, and for half an hour after we first noticed it. C. S. SCOTT.

Glatton Hall, Peterborough, June 28.

A Chimpanzee's Humour.

IN a recent lecture Mr. Romanes is reported as having strongly denied the existence of even a trace of any feeling of the ludicrous in the renowned chimpanzee "Sally." It may be worth while to record a small fact observed by me lately, tending, I think, to favour an opposite view.

Being alone with a friend in Sally's house, we tried to get her to obey the commands usually given by the keeper. The animal came to the bars of the cage to look at us, and, adopting the keeper's usual formula, I said, "Give me two straws, Sally." At first she appeared to take no notice, although she had been eyeing us rather eagerly before. I repeated the request with no further result; but on a second or third repetition she suddenly took up a *large bundle* of straw from the floor and thrust it through the bars at us, and then sat down with her back to us. Our request was perhaps unreasonable, seeing that we had no choice morsels of banana with which to reward her. She did not, however, seem ill-tempered at our presumption, and the next instant was as lively as ever. It seems to me that her action on this occasion certainly came very near to an expression of humour. Rather sarcastic humour perhaps it was, but she certainly appeared to take pleasure in the spectacle of something incongruous, and this surely lies at the base of all sense of the ludicrous. HAROLD PICTON.

July 1.

PROF. HUXLEY AND M. PASTEUR ON HYDROPHOBIA.

ON Monday afternoon the meeting called by the Lord Mayor to hear statements from men of science with regard to the recent increase of rabies in this country, and the efficiency of the treatment discovered by M. Pasteur for the prevention of hydrophobia, was held at the Mansion House. Much excellent work was done. Several letters were read from those who were unable to attend. Among these letters was the following from Prof. Huxley:—

"*Monte Generaso, Switzerland, June* 25, 1889.

"MY LORD MAYOR,—I greatly regret my inability to be present at the meeting which is to be held, under your

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